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INDEX TO VOLUME XI.

LIST OF ILLUSTRATIONS.

ERRATA.

Page 57, col. 1, 21st line from bottom, for 'thallophytes' read 'protophytes.'
" 67, " 1, 35th line from top, for 'Herndon' read 'Heudon,' as also throughout the paragraph.
" 69, " 2, 27th line from bottom, for '$2' read '$1.'
Page 96, col. 1, last line, for '108' read '118.'
" 119, " 1, 5th line from bottom, for 'homogeneous' read 'homonymous,' for 'image' read 'images,' and for 'it' read 'they.'

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AN ILLUSTRATED JOURNAL PUBLISHED WEEKLY.

Vérité sans peur.

NEW YORK: THE SCIENCE COMPANY.

FRIDAY, JANUARY 6, 1888.

THE CENTURY MAGAZINE has done an excellent thing in printing two diverse views of manual training side by side in its January issue. The expression of the two views is typical of the discussions now so prominent on this subject, and illustrates excellently why the movement in favor of manual training is so strong, and why the opposition to it is so weak. Superintendent MacAlister of Philadelphia states succinctly just what is being done in that city in the line of manual training. This summary is clear and comprehensive; his own approval of it, based on experience, is unqualified; and he tells us that manual training has won the confidence of the community. Superintendent Dutton of New Haven follows with a brief statement of the work in his city, and states that "the effect of these several forms of industrial effort upon teaching generally is good." Both of these gentlemen deal with facts with which they are perfectly familiar, and their favorable judgment upon manual training, while unqualified, is carefully and thoughtfully expressed. The gentleman who opposes manual training is Superintendent Marble of Worcester, Mass. His argument, if such it can be called, is an hysterical juggling, with more or less crude theories, and not a single fact is cited throughout his paper. An approach to a fact is the statement that a phase of manual training was tried fifty years ago, and proved a dead failure. This, however, is not true; nor, if it were, would it prove what Mr. Marble thinks it does. Manual training, as now comprehended and expounded, is not more than a dozen years old at most, and the most cursory knowledge of educational history should have acquainted Mr. Marble with this fact. The same writer talks about "the protestations of those self-constituted philanthropists," "the overthrow or subversion of the public school," "that virile quality of thought and mental power which it is the province of education to beget," "the materialistic tendency of manual training," and so on, and succeeds in demonstrating only that he is in absolute ignorance of what manual training is, and of what it is intended to accomplish. When we read a paper such as this, coming from a professed educator, it is the more easy to understand and to condone the crude speculations and outrageous theories concerning education that so often emanate from persons in no way connected with the school system of the country.

FERDINAND VANDEVEER HAYDEN.

PROF. FERDINAND VANDEVEER HAYDEN, M.D., Ph.D., LL.D., who died in Philadelphia on the morning of Dec. 22, was born in Westfield, Mass., Sept. 7, 1829. Early in life he went to Ohio. In 1850 he was graduated from Oberlin College, and soon afterward read medicine at Albany, N.Y., receiving his degree from the Albany Medical College in 1853. He did not begin the practice of medicine, but in the spring of the year of his graduation was sent by Prof. James Hall of Albany, with Mr. F. B. Meek, to visit the Bad Lands of White River, to make collections of the cretaceous and tertiary fossils of that region. This was the beginning of his explorations of the West, which continued with little interruption for more than thirty years.

In the spring of 1854, Dr. Hayden returned to the Upper Missouri region, and spent two years in exploring it, mainly at his own expense, although he was aided a portion of the time by gentlemen connected with the American Fur Company. During these two years he traversed the Missouri River to Fort Benton, and the Yellowstone to the mouth of the Big Horn River, and explored considerable portions of the Bad Lands of White River and other districts not immediately bordering upon the Missouri. The large collections of fossils he made, were given partly to the Academy of Sciences in St. Louis, and partly to the Academy of Natural Sciences of Philadelphia.

As one of the members of the Geological Survey has recently said, these collections furnished the data for profitable scientific investigation; and the researches then begun mark the commencement of the epoch of true geologic investigation of our Great West. These collections attracted the attention of the officers of the Smithsonian Institution; and in February, 1856, Dr. Hayden was employed by Lieut. G. K. Warren, of the United States Topographical Engineers, to make a report upon the region he had explored; so that the results of his labors during the three previous years were utilized by the government. This report was made in March of the same year, and in May following he was appointed geologist on the staff of Lieutenant Warren, who was then engaged in making a reconnaissance of the North-west. He continued in this position until 1859, when he was appointed naturalist and surgeon to the expedition for the exploration of the Yellowstone and Missouri Rivers, by Capt. William F. Raynolds of the Corps of Engineers of the United States Army, with whom he remained until 1862. The results of his work while with Lieutenant Warren were published in a preliminary report of the War Department, and in several articles in the 'Proceedings of the Academy of Natural Sciences of Philadelphia for the Years 1857 and 1858,' and more fully in a memoir on the geology and natural history of the Upper Missouri, published in the 'Transactions of the American Philosophical Society,' Philadelphia, 1862. This paper also included chapters on the mammals, birds, reptiles, fishes, and recent mollusca of the region in which his geological investigations were carried on. During this time also he found time to make notes upon the languages and customs of the Indian tribes with which he came in contact. These notes were embodied in 'Contributions to the Ethnography and Philology of the Indian Tribes of the Missouri River,' published in the 'Transactions of the American Philosophical Society,' Philadelphia, 1862; in a 'Sketch of the Mandan Indians, with some Observations illustrating the Grammatical Structure of their Language,' published in the *American Journal of Science* in 1862; and in 'Brief Notes on the Pawnee, Winnebago, and Omaha Languages,' published in the 'Proceedings of the American Philosophical Society,' Philadelphia, 1869.

In May, 1862, Dr. Hayden was appointed acting-assistant surgeon of volunteers by the surgeon-general of the United States Army, and was sent to Satterlee Hospital in Philadelphia. He was confirmed by the United States Senate as assistant-surgeon and full surgeon of volunteers on the same day (Feb. 19, 1863), and sent to Beaufort, S.C., as chief medical officer, where he remained for one year, when he was ordered to Washington as assistant medical inspector of the Department of Washington. On the 19th of February, 1864, he was sent to Winchester, Va., as chief medical officer of the army in the Shenandoah valley. Here he remained until May, 1865, when he resigned, and was brevetted lieutenant-colonel for meritorious services during the war. During the remainder of the year 1865 he was employed in work at the Smithsonian Institution. It was during this year that he was elected professor of geology and mineralogy in the University of Penn-

sylvania, — a position he held until 1872, when the increased executive duties in connection with the Geological Survey of the Territories induced him to resign.

In the summer of 1866 he undertook another expedition to the Bad Lands of Dakota, under the auspices of the Academy of Natural Sciences of Philadelphia, for the purpose of clearing up some doubtful points in the geology of that region, and returned with large and valuable collections of vertebrate fossils, which were described in a memoir published by the Academy of Natural Sciences of Philadelphia in 1869. From 1867 to 1879 the history of Dr. Hayden is the history of the United States Geological Survey of the Territories, of which he was geologist-in-charge, and to the success of which he devoted all his energies during the twelve years of its existence. In this time more than fifty volumes, together with numerous maps, were issued under his supervision. One of the results of his surveys, and the one in which he probably took the greatest interest, was the setting-aside by Congress of the Yellowstone National Park. The idea of reserving this region as a park or pleasure-ground for the people originated with Dr. Hayden, and the law setting it apart was prepared under his direction. The work of the Geological Survey of the Territories had its consummation in the Atlas of Colorado, which increased greatly our knowledge of one of the most interesting portions of the Great West. In 1879, after the disbanding of the Survey of the Territories, Dr. Hayden received an appointment as geologist on the newly organized United States Geological Survey. For about three years he was occupied in the completing of the business of the Geological and Geographical Survey of the Territories, and the preparation of the final results of that survey. His health had already begun to fail, but early in 1883 he asked to be relieved from the supervision of the printing of the reports, and during the three following seasons he undertook field-work in Montana. By the latter part of the year 1886 his health had become so poor that he was confined most of the time to his bed. He then resigned his position as geologist, closing an honorable connection with the government that included twenty-eight years of actual service as naturalist, surgeon, and geologist. To the general interest in science excited by the enthusiastic labors of Dr. Hayden, in his geologic explorations, is due in a great degree the existence and continuance of the present United States Geological Survey.

In 1876 the degree of LL.D. was conferred upon him by the University of Rochester, and in June, 1886, the same degree was conferred upon him by the University of Pennsylvania. Dr. Hayden was a member of the National Academy of Sciences and of many other societies scattered throughout the country. He was also honorary and corresponding member of a large number of foreign societies.

As to Dr. Hayden's personal character, those who were personally associated with him know best how genial he was, and how sincere and enthusiastic his desire to forward the cause of science. Although impulsive at times, he was generous to a fault. His subordinates all knew that each one stood upon his own merits, and that due credit would be awarded his successful efforts. The same spirit actuated him in respect to those not immediately connected with him. His views are expressed as follows in one of his earliest reports, when speaking of those who had preceded him: "Any man who regards the permanency or endurance of his own reputation will not ignore any of these frontier men who made their early explorations under circumstances of great danger and hardship."

His ideas were broad and liberal. He aimed to make a thorough astronomical, topographical, geological, and botanical survey of the Great West, with a view to the development of its mining and agricultural resources. The greater part of his work for the government and for science was a labor of love.

SCARLET-FEVER REPORT.[1] — II.

DR. R. G. Eccles of Brooklyn, N.Y., does not believe that scarlet-fever ever arises except from a pre-existent case, and says, "The following from Dr. H. B. Baker of Lansing, Mich., will help to explain some possible cases of so-called *de novo* origin: 'The Michigan State Board of Health has received information from Dr. Sifton, health-officer of Sutton's Bay Township, which illustrates in a striking way how this country gets contagious diseases from the old countries. Oct. 2, 1887, a family arrived in Sutton's Bay, Leelanaw County, direct from Norway. The family came over in the steamship "Ohio," of the Inman line, reaching New York, Sept. 30. Scarlet-fever was on board the steamer during the passage, one child dying before the landing, and "several more were sick in the same way." One child of this family was taken sick with scarlet-fever the day after reaching New York. The family, however, proceeded over the New York Central and the Lake Shore and Michigan Southern, to Michigan; then over the Detroit, Grand Haven, and Milwaukee, and the Grand Rapids and Indiana, to Traverse City; then to Sutton's Bay. Another child of the family has since come down with the disease. The family had a certificate, signed by the surgeon of the steamer, that they had been protected by vaccination against small-pox: so they passed without detention the quarantine authorities at the port of New York, after they had been exposed to a contagious disease which causes more deaths by far in this country than small-pox causes.'" He gives the following as an instance of the communicability of scarlet-fever which came under his own observation: "Arthur G., aged eight, came from the country to his Brooklyn home in sound health. A case of scarlet-fever (convalescent) being in the house upon his arrival, he was within twenty-four hours removed to other quarters, where there were no children and no disease. In a few days he had a severe attack. By perfect isolation no new cases occurred. Many such instances of short contact giving the disease have come under my observation. The best illustration my experience affords occurred during a visit I made to Wyandotte, Kan., in the winter of 1883. Mrs. S. had been visiting relatives in a distant State. In one family she called upon, they had scarlet-fever. The children were not with her. On her return home in a few days, a daughter, aged seven, was taken sick with what proved to be scarlet-fever. At this time there was not a case but itself in the town, nor had there been for many months. In their trouble, neighbors called, and within two weeks there were ten or more cases. A relative who helped them in the care of the child had three cases in his own family, he proving to be one of the victims. Two customers of his who were waited upon by him while indisposed, but not confined to bed, had each cases among their children after the exposure. No other source of contagion was possible. It must here have been carried in the clothing. Mrs. H. (my wife's mother), living in the country, visited a neighbor some miles distant, where a child was sick with scarlet-fever. A few days after the visit, her own son, aged four, who had not been exposed, was taken sick of this disease and died. There was no possible way of carrying the contagion other than upon the mother's clothes. Boards of health should require all cases to be reported to them by district sanitary inspectors, aided by physicians, the police, and the public. Their duties should be the ferreting-out of every case of contagious disease. To-day the position of inspector is a sinecure. Those holding such positions are well paid for doing almost nothing. Nearly half the cases of contagious diseases that occur, physicians do not see, nor even hear of, until some dangerous complication arises to give alarm. If they pursue a mild course, they are not heard of by the board of health, and the public schools and public conveyances scatter their virus broadcast. Conscientious physicians, too, are put at a disadvantage by their unscrupulous competitors for public favor. The doctor who is known to faithfully report every case loses his practice. People are afraid to call him, because he interferes with the progress of the children at school, and often cuts off their source of livelihood, where they carry on some industry at home. Very many physicians have boasted to me that they never report such cases unless they become so serious that they are likely to lose them. Nor can any law compel them to do so, as it is easy to introduce the claim that they had not made out a positive diagnosis. Let the inspectors, who are independent of the patients' friends, discover and report them, using all possible means as assistance."

In reference to a plan for preventing the spread of the fever, Dr. Eccles says, "The evidence we have, indicates that the germs or spores float as impalpable dust in the air. It is found by experiment that wet gauze, by evaporation, is colder than surrounding

[1] Continued from *Science* of Dec. 16, 1887.

air. Dust is attracted from warm air to a cold body. If that body is wet, it adheres. By canopies of mosquito-netting over the sick-bed, kept wet with bichloride-of-mercury solution containing glycerine, no dust can pass through the meshes in either direction. The cooled threads attract across the narrow space of the mesh all dust that reaches there. The glycerine and water fix it, and the corrosive sublimate sterilizes it. To keep up the application, two layers of netting are required, — one fixed, the other removable. The outer removable one can at stated times be wrung out of a fresh solution, and put back again. Overlapping folds can allow the passage of food, medicine, etc., to the patient. This provides perfect isolation even in a room occupied by others."

R. Harvey Reed, M.D., Mansfield, O., secretary State Sanitary Association, has known cases where old rags taken and sold from scarlet-fever cases have been used by wipers, and they in turn have communicated the disease to their families. He could give many others if it were necessary, but this fact has long since been established.

D. S. Kellogg, M.D., Plattsburgh, N.Y., believes that the disease may arise *de novo*, and bases his belief on the ground that he has had cases which he cannot *reasonably* determine, after careful investigation, originated from any previously existing case. He says, "I believe scarlet-fever to be communicable, yet last spring my belief received a severe blow. My little boy, aged six, was severely sick with this disease. My baby, aged three, slept across the hall; and my son, aged eight, slept down stairs. The sick boy was kept in a room by himself. Yet his mother and I were constantly going from the sick one to the well ones, and *not either* one of them took the disease. The sick boy 'peeled' so thoroughly that the sheets had to be shaken in order to get rid of the fine flakes of skin. He had many toys that he played with after convalescence set in. I disinfected the room in about six weeks from the beginning of his sickness, and the toys. He and the two other children have played with these toys ever since, have slept in the room for a number of months, and have not had any further scarlet-fever." He does not believe that any thing can be done by the use of remedies to prevent well persons from contracting the fever. He believes that if a person has been exposed to scarlet-fever, the better his physical condition, the better is he able to endure the disease. There are many instances that would make this not seem true.

T. D. Crothers, M.D., Hartford, Conn., says, "In 1868 I traced in an epidemic twenty-one cases to contagion clearly. The communicability was by contact in most cases; in others it was through the near association. In two instances a linen picture-book was the medium of communication of the poison. In several cases it was taken by the clothing of persons who had been nursing such cases. Clothing has retained this infection several weeks when confined in a trunk. Many cases have occurred in a community, and been confined to a single case by means of isolation, quarantine, disinfection, and extreme cleanliness."

William H. Brewer, professor in Sheffield Scientific School of Yale University, New Haven, Conn., in reply to the question whether scarlet-fever ever arises *de novo*, says, "There are insufficient data for a *positive belief*. From the evidence, however, that we have, I say *no*, until better evidence is brought forward that it does arise *de novo*. Quarantine the cases if public opinion will justify: if not, then the first duty of the board is to educate the public as to the facts and the dangers. So soon as the public is ready for it, scarlet-fever will be more rare than the small-pox. But before this can be brought about, there must be a strong public feeling that it is a controllable disease."

W. C. Van Bibber, M.D., Baltimore, Md., thinks that boards of health should endeavor to change the non-sanitary condition of neighborhoods and places; for, although scarlet-fever may not now be fairly numbered among the filth-diseases, yet cleaning and sanitary laws may do good on general principles. Cleaning, segregation, and belladonna internally, ventilation, and increased vigor by increasing the vigor of individuals, should be employed. He says, "I attended Christ Church Charity School, Baltimore, for thirty-six years. The means above mentioned were used where a case of scarlet-fever occurred. The school consisted of thirty-two children. In thirty-six years there was but one death. The disease appeared in the school more than twenty times, and was always kept confined to but few children by means of these precautions. By personal hygiene, continued life in open air, the use of belladonna internally to those exposed, and rubbing the diseased body with disinfectants, much may be done to prevent the spread of the disease. I combine in an oil embrocation (thymol, anise-oil) carbolic and salicylic acids, and camphor.

DO FORESTS INFLUENCE RAINFALL?

IT is very generally believed that the culture of forests induces an increase in rainfall, and that their destruction diminishes it. A satisfactory explanation of this supposed phenomenon has never, as far as I am aware, been offered; and the only tangible support for the theory appears to consist in a few observations of rainfall in limited areas in central Europe, made before and after reforesting. It seems desirable that the question should be tested by all the evidence at hand, and the theory established or disproved by the facts. We have in this country the material for testing both phases of the theory upon a large scale and in an exhaustive manner.

The prairie region, including Iowa, northern Missouri, southern Minnesota, most of Illinois, and a small part of Indiana, has, during the past thirty years, undergone a great change with respect to its vegetation. This great area of over 100,000 square miles, was, when settlement commenced, mainly grass-covered. It contained no forests. Belts of trees were found along the water-courses, upon the slopes of river-bluffs, and here and there upon the slight elevations. But man has encouraged the growth of trees, and the area of arborescent vegetation has been greatly increased. It is an example of reforesting upon an immense scale, unequalled elsewhere upon the globe. Has the rainfall correspondingly increased?

The early settlers in Ohio found it mainly a forest-covered region. It has been remorselessly cleared. This area of 40,000 square miles does not contain to-day a tithe of the timber-land that it contained fifty years ago. Has the rainfall diminished?

The States of Massachusetts, Rhode Island, and Connecticut, with adjacent parts of New York, New Hampshire, and Maine, — an area of perhaps 25,000 square miles, — were, when Europeans entered them, densely covered with forests. In time these were almost entirely cleared away. In recent years, however, a change in the occupations of the people of this densely settled region, in virtue of which the farms are being abandoned, while the inhabitants are becoming massed in the cities, has allowed an enormous increase in the wooded area of these States. To-day at least half this area is again covered with woods.

If this theory be correct, the rainfall in this region should have diminished from the colonial times down to, say, 1860, while since that date it should have been on the increase. Are these the facts?

We have here three areas of considerable magnitude, in which radical changes in the forest-covering have been made during the present century. Fortunately, also, we have ample records of the rainfall during these periods.

First, however, a word as to the character of the rainfall. Of all current meteorological phenomena, rainfall is the most irregular, both as to time and place. The rainfall of one year may be double or treble that of the year before or the year following. At any one station these fluctuations are ordinarily so great as to thoroughly mask any secular change. It may vary greatly from place to place, even though the distance be small, while the change of the location of a gauge from the ground to the top of a house may make it give very different indications. For these reasons it is apparent that reliable results, in regard to a general increase or decrease of rainfall, are to be obtained only by combining a large number of observations scattered over many years and over the greatest possible variety of conditions. It is a very easy matter to so select stations, and years of observation, as to obtain any pre-arranged result.

If there has taken place a change in the amount of rainfall in any or all of these regions, it must, in the nature of things, have been a progressive one, however disguised by sporadic fluctuations. Moreover, if this increase or decrease in rainfall produces the results claimed for it, making a desert fruitful, or the reverse, it must

be of considerable magnitude, sufficient to be expressed in inches, annually.

In the prairie region I find twenty-four stations at which extended series of rainfall measurements have been made. None which have been used are less than ten years in duration, and they range thence up to forty years. The sum of all these series is four hundred and twenty-eight years. Each of these series was divided into two equal parts, and the total rainfall of each half obtained. On the theory of a progressive increase, the sum of the second half of the series should be greater than that of the first half. The following table exhibits the result. The first column gives the names of the stations; the second, the length of the series; the third, the total rainfall in the first half of each series; the fourth, the same for the second half; and the fifth column, the differences between them, an increase having the plus-sign, a decrease the minus-sign.

Prairie Region.

STATIONS.	YEARS.	AGGREGATE RAINFALL.		DIFFERENCES.
		1st half.	2d half.	
Chicago	22	348	420	+72
Athens	16	332	299	—33
Augusta	18	352	341	—11
Dubois	10	221	186	—35
Galesburg	10	175	158	—17
Manchester	18	335	325	—10
Marengo	16	354	269	—85
Ottawa	14	266	235	—31
Peoria	18	322	296	—26
Riley	14	269	237	—32
Sandwich	12	323	242	—81
Winnebago	14	271	239	—32
Wyanet	10	195	191	—4
Springfield	30	704	763	+59
Dubuque	18	293	317	+24
Omaha	16	235	318	+83
Leavenworth	18	367	363	—4
Davenport	22	463	406	—57
La Crosse	24	418	412	—6
Milwaukee	40	611	657	+46
Brookside	10	232	250	+18
Fort Madison	26	569	457	—112
Iowa City	14	306	282	—24
Muscatine	18	414	369	—45

The results, as will be seen, have a wide range, some stations showing an increase, while much the larger number show a decrease. Now, although these series overlap one another in all sorts of ways, and do not necessarily refer to the same years, still, under the theory of a progressive change, they may be combined directly without involving error. We may add up columns 1 and 2 and strike a balance, and this balance shows a greater rainfall in the first period by 343 inches. Dividing this by the number of years in the period, 217, it is discovered that on an average each station received per year 1.58 inches more rain during the first period than during the second: in other words, instead of an increase of rainfall being produced by the increase of arborescent vegetation, there has occurred, from some cause, an actual diminution. I should be very slow to argue from this a deleterious action flowing from the increase of forests, but it seems to militate very strongly against a favorable action upon rainfall.

In Ohio the contrary result is to be sought; viz., a decrease in rainfall owing to the destruction of forests. In this State I find twelve stations, with series ranging from ten to forty-eight years each, and an aggregate of two hundred and ninety-four years. The observations have been treated as were those in the prairie region, with results as given in the following table: —

Ohio.

STATIONS.	YEARS.	AGGREGATE RAINFALL.		DIFFERENCES.
		1st half.	2d half.	
Cincinnati	46	1044	965	—79
Cleveland	28	513	531	+18
College Hill	14	329	319	—10
Hillsboro'	16	329	307	—22
Hudson	12	203	241	+38
Kelley's Island	10	166	159	—7
Marietta	48	1005	1033	+28
Portsmouth	26	475	547	+72
Steubenville	40	807	836	+29
Toledo	22	412	364	—48
Urbana	18	353	333	—20
Waterville	14	275	245	—30

It will be seen that in this case the total rainfall of the first half of the series is slightly greater than that of the second half, the difference being 31 inches, which, divided by the number of years in the first half of the series, shows, that, along with the clearing of the forests, the rainfall has diminished a trifling amount, being 0.21 of an inch less in each year of the second period than the first. It is, of course, unnecessary to add that this change is too small to have any meaning.

In the third area, that of southern New England, there is to be expected a diminution of the rainfall, consequent upon deforesting, which was in progress down to, say, 1860, and, in more recent times, an increase due to reforesting. Prior to 1860, I have eighteen series, ranging in length from ten to forty-six years, with an aggregate of four hundred years. Treated as before, the results shown in the following table are obtained. Summed up, they show that the aggregate rainfall in the second period was greater by 579 inches, or 2.9 inches in each year of the period. Deforesting, in this case, seems to be accompanied by a decided increase in rainfall.

New England.

STATIONS.	YEARS.	AGGREGATE RAINFALL.		DIFFERENCES.
		1st half.	2d half.	
Amherst	24	506	550	+44
New Haven	20	456	453	—3
Boston	34	689	723	+34
Cambridge	20	435	491	+56
Lowell	12	267	274	+7
Lunenburgh	20	493	544	+51
New Bedford	46	978	958	—20
Waltham	10	231	212	—19
Worcester	20	435	523	+88
Fort Adams	12	294	273	—21
Providence	28	539	613	+74
Flatbush	26	555	555	0
Albany	26	527	528	+1
Jamaica	22	402	413	+11
New York	10	211	246	+35
West Point	20	466	486	+20
Brunswick	30	604	748	+144
Gardiner	20	379	456	+77

Subsequent to 1860, I have fourteen series, ranging in length from ten to twenty-four years, with an aggregate of two hundred years. The results, presented below, show that the rainfall in the two halves of these series was identical.

Stations.	Years.	Aggregate Rainfall.		Differences.
		1st half.	2d half.	
Amherst	14	318	310	−8
New Haven	14	347	348	+1
Boston	24	597	572	−25
Fort Trumbull	10	241	229	−12
Middletown	14	324	338	+14
Lawrence	12	279	265	−14
Lunenburgh	14	313	343	+30
New Bedford	14	300	348	+48
Providence	16	377	393	+16
Albany	16	328	305	−23
Flatbush	12	234	237	+3
New York	16	373	382	+9
West Point	10	246	209	−37
Gardiner	14	305	303	−2

With these results in view, it seems idle to discuss further the influence of forests upon rainfall from the economic point of view, as it is evidently too slight to be of the least practical importance. Man has not yet invented a method of controlling rainfall.

HENRY GANNETT

THE GERM THEORY AS A SUBJECT OF EDUCATION.

THE time is past when it is necessary to discuss the probability of the 'germ theory' as explaining infectious diseases. This is no longer a theory, but as fully demonstrated as most of the other universally accepted conclusions of science. No one to-day who is competent to form a judgment from a knowledge of the facts, will doubt that many infectious diseases are caused by the growth of microscopic organisms in the body. Of course, no general proof of the parasitic nature of all infectious diseases has been adduced, nor is such general proof possible; but when the causal connection between certain specific bacteria and definite infectious diseases has in many cases been proved by a demonstration so conclusive as to be beyond question, and when such causal connection has been rendered extremely probable in many other cases, indeed in almost every infectious disease, it is only ignorance of the facts that can explain any doubt as to the very general applicability of the theory. It is true that many, perhaps a majority, of practising physicians do not have much sympathy with the conception of the parasitic nature of infection, sometimes indeed treating the whole subject with ridicule. Some are incapable of forming correct judgments, but most of them have not found the time or inclination to study the subject enough to know what facts have been established. At the time when most of the physicians who are now practising were pursuing their studies, the germ theory of disease was scarcely entertained as a theory, and nowhere accepted. Only three or four years ago some of our better medical schools taught their students that the theory was a wild hypothesis, and destined to be exploded like any other visionary speculation. It is not surprising, therefore, that they should still refuse to accept a theory which so revolutionizes the conceptions of disease. But our leading physicians, including professors in better medical schools, are now convinced of the truth of the theory and the great importance of the subject, and medical papers throughout the country are giving more and more space to the subject of bacteriology. The inevitable result of this will be that the next generation of doctors will accept the germ theory as the basis of practice.

This discovery of the parasitic nature of infectious diseases is of more than scientific interest: it is of vast practical value. It has not yet, perhaps, contributed very materially to the methods of treating disease directly, although we may confidently expect great results in the future along this line. There is nothing to prevent direct experiments with germicides upon living bacteria in the laboratory, and we may hope in this way to get a more scientific method of curing infectious diseases, after the theory of their parasitic nature becomes more truly the property of doctors as well as of scientists. Thus far, however, the value of the theory has been rather as the foundation of the science of preventive medicine. Here its importance cannot be overrated, and is only beginning to become realized.

We need do no more than mention the advances made in surgery in the last twenty years, which are due almost solely to the knowledge of septic bacteria. It needs no words to enforce the value of discoveries in this line. Every one appreciates this matter; and the value of antiseptic dressing, which alone makes the difficult operations possible, is almost everywhere recognized, and its use taught in all medical schools.

In other lines than surgery the value of the germ theory is even greater, though at first sight not quite so apparent, since the matter is yet in its infancy. The great advantage which we are to acquire through this theory is not in curing infectious diseases, but in preventing them. Professor Koch, in a recent address to a class of medical students, voiced this fact: "Gentlemen," he said, "you have been hitherto taught only how to cure disease, in the future you will be taught how to prevent disease." We can see in this direction great practical results arising along at least two different lines. The first is by preparing the body to resist the disease, the method of inoculation. The most widespread instance of this method of treatment is of course vaccination for small-pox. Vaccination was discovered, it is true, empirically, and entirely independent of the germ theory; but it finally received its *rationale* through the brilliant work and generalizations of Pasteur. Working in accordance with the same idea of preventing a severe form of a disease by giving the individual previously a mild form, Pasteur has successfully treated splenic-fever and hydrophobia. Others, following in his lead, claim success in a similar treatment of yellow-fever and cholera, although these claims certainly need further verification. But only a beginning has been made in this direction, and it does not seem improbable that we may see a time when many of our most severe epidemics may be as thoroughly subdued by inoculation as small-pox has been by vaccination.

But of much more importance than inoculation is the more natural method of avoiding the diseases. We are now learning to keep the bacteria away from our bodies, either by directly destroying them or by keeping away from the contaminating material. When we know the exact nature of an infectious disease, — what are the habits of the organism which produces it; where they are most likely to be found lurking during epidemics, whether in water, food, clothing, drains, in the air, in the excreta or scales from the skin of the patient; in what conditions they will grow, and what will kill them; how they make their way into the healthy body, whether by food, drink, by breathing, or by contact of infected material with the skin, — in short, when we understand the natural history of an infectious disease, it is usually easy to avoid it. If the disease is taken in drinking-water, it may be avoided or rendered harmless; if in food, the food may be cooked; if from excreta or clothing, they may be easily disinfected by some of the effective germicides; if by contact with the skin, care in handling the infected material, and disinfecting the skin afterward, will usually suffice. As yet we have discovered no way of avoiding contagion which comes to us in the air, but we are just beginning to find out the extremely important fact that the air does not become contaminated with bacteria unless they are allowed to dry. Recent investigations have shown a smaller number of bacteria in the air of a well-kept sewer than in that of a poorly ventilated schoolroom. It is a valuable discovery that this means of infection by breathing — a means which we cannot guard against — is uncommon. The air is not the ordinary mode of transferrence of germs, and would be scarcely at all, if proper precautions were taken to prevent infectious material from drying. Here we immediately get suggestions as to the management of the

hospital and the sick-room, and as to general sanitary measures, which will enable us to stamp out many of our most dreaded diseases. How suggestive to remember the experience of Professor Koch and his associates! While at Alexandria, although surrounded by the cholera epidemic, they had no difficulty in avoiding the disease by the adherence to certain precautions which a knowledge of the germ nature of cholera had taught them; but upon return to Germany, and being thus many hundreds of miles from the disease, one of them acquired the disease by a careless handling of the cholera germs which they had brought with them. What better proof could there be of the value of knowledge of the facts? By study of bacteria we are beginning to understand why one disease is contagious and another not contagious, or why a third disease may be sometimes contagious and at other times not at all so. We are learning what are the sure and what the worthless methods of disinfection. Thus the mysteries connected with infectious diseases are disappearing.

It is not of very much value to know the simple fact that a particular disease is parasitic in its nature, unless this is made the basis of further intelligent observation. Nor does it help us any, as Dr. Hunt recently pointed out in this journal, to be able to distinguish the specific germ producing any disease if we end our observations with this discovery. It is of great value, however, to know the habits of the microbe and the conditions in which it can live, and these facts can only be discovered by the study of the microbe itself. This is the share which the biological laboratory must have in the matter. It is of course necessary to study the disease itself, and the conditions under which it propagates itself, with vigor; to study the origin of epidemics, their spread and decline; but this can only be done intelligently when we understand the nature of the organism producing it. When we know the habits of a microbe, — whether it lives in acid or alkali solutions, whether in filth or cleanliness, whether best in heat or cold, etc., — then we can successfully ask questions concerning the conditions in which the disease develops; then we can discover the history of the organism from the time it leaves the body of the sick person until it gets into a second individual and again produces its disease; then we can learn what conditions favor and what hinder the disease; then we can discover how to prevent this transferrence, how to kill the microbe in its passage; and then we shall have gone far toward ridding the race of our vigorous epidemic diseases. Sanitary measures need no longer be blind methods applied tentatively, but may proceed directly at the root of the disease from a knowledge of its cause. Sanitary science must indeed be founded upon the knowledge of the nature and habits of microbes.

Advance along these various lines of preventive medicine has been rapid in the last few years, and is becoming more and more so, and chiefly through the study of facts discovered in connection with the growth and distribution of microbes. Although many questions still remain unanswered, the knowledge of the parasitic nature of infectious diseases is enabling doctors and scientists together to ask intelligent questions concerning such diseases, and to search for their answers in the right direction. Until this knowledge had appeared, such questions and researches could only be made at random. In short, the knowledge collected concerning the parasitic nature of disease and the habits of the specific microbes is giving us hundreds of ways of fighting the diseases outside of the body, even though it has yet not been very fruitful in directing our physicians how to treat the disease when it has once vigorously attacked the body.

The importance of a general understanding of the facts connected with the discoveries in this direction cannot be overrated. Who is there, old or young, who would not be benefited by a knowledge of the source and cause of infectious diseases? Who is there who is not better prepared for life by a knowledge of what is meant by cleanliness, and why it is so desirable, particularly in time of epidemics, to keep our surroundings perfectly clean? Ought not every one understand as far as possible where the infectious organisms are likely to be, and how they may be avoided? Indeed, is not this subject one of the many which we are beginning to recognize as desirable in our public-school teaching? Physiology is taught now in our schools by law, but what branch of physiology can be of more value to the public than a few principles connected with infectious diseases, and the means of keeping contagion away from our doors? If physiology is to be taught in the schools, would it not be well to include in it some such principles of vital importance, instead of compelling the student to learn the names of the bones in the body? At present the public gets informed in such matters only through the uncertain medium of the press, which contains as much false science as true; and as a result it is almost impossible rigidly to enforce sanitary measures. It is needless to say that the public schools have not yet taken up the subject. Our colleges, too, ought to see that every student knows something of this matter. A few of them already realize the fact, and have made a beginning in this line. Our training-schools for nurses ought certainly to put much force upon this subject and the practical precautions connected with it. But, after all, we must look primarily to our medical schools for teaching in this direction. Doctors will always be regarded as authorities in matters connected with health, in spite of nurses or the sayings of scientists; and it is through them that the public must receive its education. The medical schools must therefore lead in this matter. It is true that medical schools aim to teach chiefly how to cure disease, and as yet the germ theory has not materially aided in this direction. It is of course difficult to find time, in the already crowded course, to introduce any new subject not directly related to the cure of disease. But bacteriology is a subject too important to be neglected: it readily forms a part of pathology, and most schools do find time for a treatment of this subject. Our medical schools are now pushing on in this direction. Two or three years ago the theory was dismissed with a word, even in our best schools; and that word was frequently one of ridicule. Now many of the leading medical schools pay considerable attention to the subject. Several of them have among their faculty special bacteriological students who give instruction in this line. A few have well-equipped bacteriological laboratories, and others are looking in the same direction. To what extent the subject is treated in the medical schools of the country in general, or in the training-schools for nurses, cannot be stated at present. Inquiries are being set on foot in this regard, the results of which will appear in some future numbers of this journal.

H. W. CONN.

AMERICAN SOCIETY OF NATURALISTS.

THE annual meeting of the American Society of Naturalists was held in the Peabody Museum, New Haven, on Dec. 27 and the two following days. The object of the society is to help instructors in the natural sciences by discussing the methods of research and of instruction. Leaving to the other scientific associations the function of presenting and discussing results, this society, composed of professors and specialists, devotes itself to the publication of new methods, improved apparatus, and aids to science-teaching, all of which are apt to be scattered through various periodicals, and thus fail to secure that general adoption which a practical demonstration of their usefulness would bring about. The work of the society falls into two sections, — biology and geology, — and a day of each meeting is devoted to each of these topics, while the third day is given over to a general discussion upon some attractive subject. The society, though in existence only for a very few years, has a large membership, including in its list many of the eminent leaders of science in this country and in Canada. The attendance at the recent meeting was quite large, and the proceedings both interesting and profitable.

The proceedings were opened by the address of the president, Dr. Harrison Allen of Philadelphia. His subject was 'The Inconstant in Biology,' and was devoted to the discussion of variations in animal structure not easily referable to any law, but to which careful study would attach considerable significance. In particular, he called attention to the prevalence of hairy parts and of color-spots in animals that had to a greater or less extent deviated from their normal type. If, for example, a variety broke from the prevailing color of its kind, the original color would be retained at certain very definite spots: these are found at the tip of the tail, another around the eyes, a third on the skin covering the dorsal column, and elsewhere. The peculiar constancy of these places of

retention of the original color was especially emphasized, and the inference drawn that here was something too deep for natural or other selection to weed out, and the explanation of which would be a valuable contribution to the history of animal life. Dr. Allen illustrated his propositions with a series of mounted specimens, and brought out an interesting discussion upon color-markings in general.

Dr. Oliver exhibited a series of carefully prepared wools for the detection of color-blindness and of sub-normal color-perception. Professor Gage described an easy method of injecting the thoracic duct and of demonstrating it for students. Professor Osborn exhibited some sections of the brain and spinal cord prepared by a method that allows of more accurate work than has hitherto been possible. Dr. Minot exhibited a new microtome of his own invention, for which he justly claimed some important advantages. In this the knife is stationary, and by a simple motion of a wheel the thinnest sections can be automatically cut as accurately and as rapidly as desirable. The instrument will be supplied by the Educational Supply Company, Boston. Prof. H. N. Martin showed a very simple device by which either the closing or the opening shock could be separately used for stimulating nerve-muscle preparations in physiological work. A very interesting paper was that of Prof. S. F. Clarke, presenting a classroom demonstration of variation in nature and under domestication. By a series of stuffed fancy pigeons the very varied and fantastic forms of variation that the will of man could bring about was most beautifully impressed; and in striking contrast to this was a series of sparrows, the distinction between which required the closest observation, but which represented no less than eight genera and thirteen natural species. Prof. E. S. Morse, with his usual happy manner, *résumèd* the kinds of museum show-cases employed in Europe, and accented the points of value in each.

To the general student of science the discussion upon science-teaching in the schools, to which an entire day was devoted, would form a most interesting feature of the meeting. The discussion was introduced by Prof. Ramsay Wright of the University of Toronto, who briefly sketched the admirable system of science-teaching in the schools of Ontario. Here the whole educational system is in charge of a minister of education, who has at his service the advice of the university professors, and who, with their aid, has drawn up a schedule of instruction in science which is utopian compared with any thing that exists in the schools of this country. Here the fact that the government controls the granting of certificates and the appointment of teachers has solved the problem of securing able science-teachers for the schools.

Prof. Alexander Winchell of the University of Michigan followed with a forcible plea for the educational value of the study of geology. He claimed for this study the discipline of all those powers of the developing mind upon which a true culture was based. In the child, observation, training of the senses, was the first natural process; and this it was, too, that geology first demanded. With the growth of mental powers came the wider field for their employment, in the induction of the general geologic principles from the observed facts, in the grand deductions from these, and in the exercise of the imagination that geological periods make necessary. He would thus urge the teaching of geology in the elementary schools; and, because this study afforded such varied opportunity for the exercise of all the faculties proportionate to the natural order of their development, he thought it proper to speak of a geological culture.

The next contribution to the discussion was by Professor MacCloskie of Princeton College. He urged in a very emphatic manner the rights of science as opposed to the word-knowledge and the language-culture, that absorbs so much of school time and energy. While the position urged was not a new one, it very forcibly expressed the independent right of science to a very early and important place upon the curriculum of every school. The discussion was concluded by Professor Rice of Wesleyan College, who presented a masterly exposition of the theoretical and practical advantages of science-teaching in the schools. The boy or girl that has not been spoiled by artificial means is invariably interested in the phenomena of nature surrounding him or her on all sides. It is with reference to these that their questions are asked, and it is in the observation of these that they find a satisfaction of their natural curiosity. The current methods of teaching in large measure crush this natural interest, and substitute for it an unnecessarily stupefying word-drill. The result of this is that young men come to the higher schools with a total lack of appreciation for the world of natural fact, and, what is worse, a dulling of all the faculties by which such an appreciation can be attained. It is not the facts of science, but the appetite of the mind for this kind of knowledge, that is to be ever kept awake, and without which that new sense for the teachings of nature cannot be fostered.

All these papers brought out an animated and profitable general discussion from various members. The sense of the meeting was unanimously in favor of the views expressed above; and the advantages of introducing science into the elementary schools was urged not only for its practical value, but for its satisfying the requirements of the natural growth of mind and its general disciplinary value. That children properly trained to an interest in the affairs of science do really bring to their more mature years an appreciation for true science, and the ability to carry it on to a high grade of cultivation, has been proved more than once. On the practical side the question of the order of the sciences in school-work was discussed, and the general opinion was in favor of systematic botany as the topic with which to begin, then physiology, and then physical geography. A complete course in physiology, however, must be based upon some knowledge of physics and chemistry. A committee was appointed to consider the preparation of a schedule of science instruction for the schools, and was authorized to report in full at the next meeting of the society.

The geological part of the proceedings was opened with a paper by Prof. James D. Dana, who recounted some of his recent observations on the Hawaiian volcanoes with especial reference to the connection between seismic phenomena and lava eruptions. The rarity of explosive action, so common in most volcanoes, is well known to be the most distinguishing feature of the Sandwich Island craters. The mountains are nearly pure lava-cones, and the eruptions are fissure eruptions. In only two of the numerous recorded outbreaks, viz., in those of 1868 and 1887, have earthquakes of any violence been noticed. These shocks increased regularly in intensity, and were abruptly terminated with the appearance of the lava. Professor Dana concludes that they were produced by the forcible rending of the solid crust, caused primarily by the vapor tension from water heated from the outside of the lava-conduit; and secondly by the hydrostatic pressure of the lava itself within the conduit. In most cases the formation of the fissure through which the lava is extruded is accomplished so quietly that the first intimation of an approaching eruption is the red glow of the molten mass. In conclusion an interesting comparison was drawn between the quiet type of lava-flow prevalent at the Sandwich Islands and the violently explosive outbursts like those recently exhibited in Java and New Zealand.

A paper by Mr. C. D. Walcott of the United States Geological Survey described an ingenious method of measuring the thickness of inclined strata.

Professor Dwight described an admirable machine, devised by him, for cutting large sections in any plane through fossils. For this purpose a Kerr diamond saw is mounted horizontally, and held rigidly in a plane by two disks carrying small wheels which are in contact with both surfaces of the saw. The specimen to be cut is mounted and adjusted so as to bring any plane against the saw with an even pressure. A solution of soda was recommended as a lubricator.

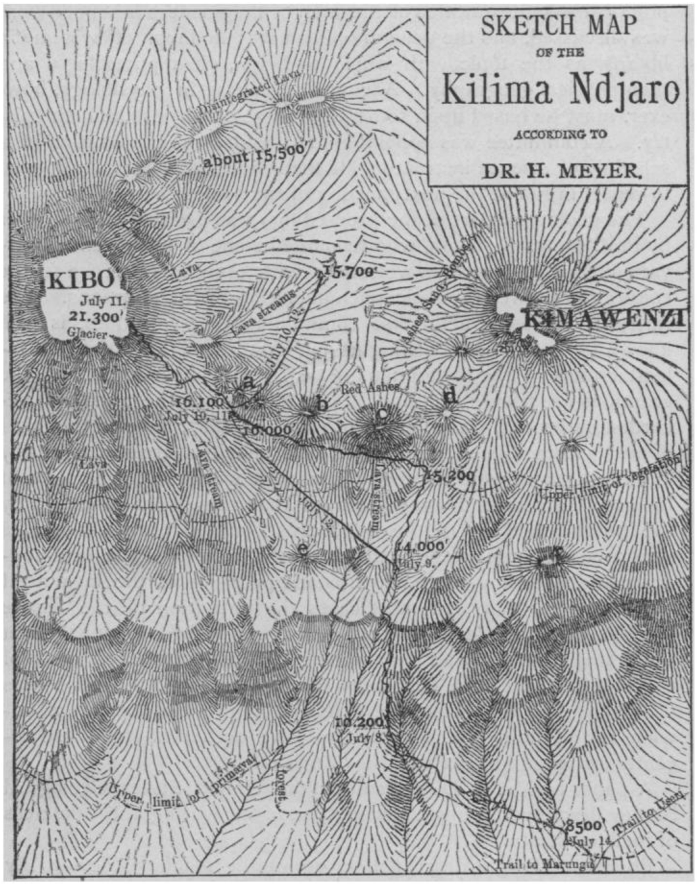
Prof. W. O. Crosby sent a paper upon the method of teaching mineralogy and lithology at the Massachusetts Institute of Technology. The last paper was presented by Dr. G. H. Williams, on the educational value of micropetrography, and illustrated by the exhibition of a new microscope of American manufacture, designed especially for students in this subject.

A resolution was passed to request Congress to remove the duty on scientific books and apparatus, and to join with other associations petitioning for this change in the laws. Professor Marsh was elected an honorary member of the society, to fill the vacancy left by the death of Professor Baird. Dr. Allen was re-elected president, and Professor Clarke secretary. The next meeting will be held in Baltimore.

EXPLORATION AND TRAVEL.

Dr. H. Meyer's Ascent of the Kilima Ndjaro.

IN a letter to the Geographical Society of Leipzig, which has been published in *Petermann's Mitteilungen*, Dr. H. Meyer describes his ascent of the Kilima Ndjaro. On July 2 he left Taweta, and, after a two days' march through steppes and brushes, he reached, in company with Herr von Eberstein, the village of Mareale, a chief of the Marangu. He was kindly received, and Mareale gave him three guides, with whom and twenty-two men of his caravan he started for the Kibo, the higher summit of the Kilima Ndjaro. At a height of 5,700 feet they passed the last plantations of bananas, and entered the primeval forests, which are always full of mist, at a height of 6,600 feet. After two days, having passed these forests, they reached the grassy belt surrounding the upper part of the mountain. Here they left the trail which leads along the south-eastern slope of the Kimawenzi to Useri, and turned north-westward, following the upper limit of the forests. At the end of the second day they reached the place where, in 1884, Johnston had staid for some time, at a height of 9,800 feet. Here part of the caravan remained, but eight men volunteered to carry tent, blankets, instruments, and provisions to the snow-line. The route led over grass-covered streams of lava, which were intersected by gulches of 150 feet depth, cut by the torrents which come from the snow-fields of the summit. From here the saddle between the Kimawenzi and Kibo appears almost horizontal. The travellers ascended a lava-stream, and soon reached a gently sloping region where meadows indicated the course of the brooks. Here the first patches of snow were met with, and Meyer left here his tent on the 9th of July, at an elevation of 14,000 feet.



On the 10th, when Meyer intended to strike camp, five of his servants refused to accompany him any farther, and therefore they were left behind, while the rest of the caravan continued their march. After a short time they reached the steep, fissured hill *c*, from which the lava-stream had come on which they travelled the preceding day. Here they discovered the series of parasitic craters *a*, *b*, *c*, *d*, from which numerous lava-streams have flowed southward. Some of these are separated by deep valleys, while others form a continuous plateau which stretches out far northward between the Kimawenzi and Kibo. Meyer proceeded near the southern part of these hills at a mean elevation of 16,000 feet, and made his last encampment at the foot of the hill *a*. As the night promised to be very cold, he sent his three negro servants back to the previous camp, and ordered them to return the next day. Thus he and Von Eberstein were alone, and passed the following night at a temperature of —11° C. (12° F.) in their small tent. After a careful examination of the cone of Kibo with a spy-glass, Meyer concluded that an ascent on the south-eastern side was possible. But on the highest summit a light blue wall of ice was seen, which extended to a lower level on the south side of the mountain. In the beginning of the next day, after having passed lava-streams covered with large bowlders, the travellers reached continuous steep snow-fields filling the rounded valleys between enormous lava-streams. Johnston had reached this point, and a little farther to the north Count Teleki had attempted an ascent a few weeks before Meyer's arrival. In the morning the weather was clear, the snow hard, and therefore the travellers succeeded in reaching a considerable elevation; but after three hours' climbing, fog set in. In the beginning the mist was light, and the summit of the mountain could be seen occasionally. Wherever a lava-stream crosses an older one, a new snow-field begins, steeper than the preceding. At such points the travellers staid for a few minutes, making barometrical observations and collecting rock specimens and lichens. They were careful not to ascend too rapidly, as work in elevations of more than 17,000 feet in height is extremely exhausting. Later in the day the fog became thicker, the highest parts of the mountain became invisible, and the sun disappeared. The temperature fell from 8° C. (46° F.) to —3° C. (27° F.), and a snow-storm set in, which threatened to obliterate the track. About half an hour later, Herr von Eberstein began to fall back, and after a quarter of an hour more his strength left him. As they were not far distant from the rim of the crater, Meyer proceeded alone, and notwithstanding giddiness, breathlessness, and exhaustion, succeeded in ascending the last steep snow-field. Here the slope became less steep, and, after having climbed over a field of gigantic bowlders of ice, he reached the ice wall which he had sighted from the last camp. It is about 100 feet high, and inaccessible without the help of several expert guides and a great apparatus of ropes, ladders, etc. Although Meyer did not reach the rim of the crater itself, he concludes that it is probably filled with ice, as the ice wall projects over it on all sides. After having observed the barometer and thermometer, he returned to where he had left Von Eberstein, who had meanwhile observed the boiling-point thermometer. After a rest of about a quarter of an hour, they continued their descent, and reached their tent after an absence of seven hours. On the following morning the northern part of the saddle was visited for making topographical observations, and, after the three negroes had returned, the party continued their descent of the mountain, and reached Mareale's village after a march of four days.

NOTES AND NEWS.

ON Friday, Dec. 30, a meeting was held at the College of Physicians and Surgeons in New York City for the purpose of organizing an American physiological association. The association has for its object the promotion of physiological research and of social intercourse among the physiologists of the country. The association will meet as a section of the Medical Congress every three years. The meeting was presided over by Dr. S. Weir Mitchell and many prominent physiologists from all parts of the country were present. A constitution was adopted, and Prof. H. P. Bowditch of the Harvard Medical School was elected president, and Prof. H. N. Martin of Johns Hopkins University, secretary and treasurer.

— The seventh course of free lectures of the Cincinnati Society of Natural History will be given on Friday evenings in January February, and March, 1888, in the rooms of the society. The following is the programme: Jan. 6, Charles B. Going, 'How the Chemist Works;' Jan. 13, George Bullock, 'Modern and Orthochromatic Photography applied to Natural History;' Jan. 20, B. Mer-

rill Ricketts, 'The Dermal Coverings of Animals and Plants;' Jan. 27, Joseph F. James, 'The Great Deserts of the Earth;' Feb. 3, Amos R. Wells, 'Volcanoes;' Feb. 10, D. S. Young, 'Some Characteristics of Fishes;' Feb. 17, Charles Dury, 'Reason and Instinct in Animals;' Feb. 24, Walter S. Christopher, 'Bacteria and Fermentation;' March 2, F. W. Langdon, 'Races of Man;' March 9, A. B. Thrasher, 'The Voices of Animals.'

— The Council of the American Economic Association held its annual meeting in Hamilton Hall, Columbia College, at 10.30 A.M., Friday, Dec. 30.

— The *Political Science Quarterly* for December contains several articles that are worth reading, though none of special importance. Two of them are on the subject of profits and wages, — a subject that is sure to attract readers, but on which we cannot say that much light is shed. Professor Clark recognizes the fact, which most economists overlook, that a large portion of the employer's profits is of a mercantile character, arising from buying and selling to good advantage rather than from special skill in production; but, strangely enough, he thinks that this profit is due to causes beyond the employer's control, and "comes to him as rain from the clouds;" whereas it is due in great measure to his skill in taking advantage of the markets so as to buy at a low price and sell at a high one. The opening article of the number is a vigorous attack on the oleomargarine law, and will be read with interest by all opponents of government interference. The article on local government in England is of interest just now, when new and extensive changes in that branch of the English Government are in contemplation. There is also an article of considerable historical interest, on the Constitution in reconstruction, giving an account of the contest between Congress and President Johnson in regard to the recognition of the Southern States and the guaranties to be required of them before such recognition was granted. The closing essay is on India's unadjusted trade balance, and the usual complement of book-reviews fills up the number. This review, together with the *Journal of Economics* issued at Harvard, and the various publications of the Johns Hopkins University, are an addition to our periodical literature; for they furnish a kind of reading that we should otherwise hardly get.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Twenty copies of the number containing his communication will be furnished free to any correspondent on request.

The editor will be glad to publish any queries consonant with the character of the journal.

The Flight of Birds.

IN your issue of last week, my friend, Dr. Elliott Coues, takes part in the current discussion of the flight of birds with his usual boldness and independence, but not with his usual care and accuracy. He practically begins his letter with the following *ex cathedra* condemnation of Professor Trowbridge's theory, and denial of his facts: "With regard to the alleged locking of the primaries: 1. It does not take place; 2. Did it take place, flight would be impossible."

As Professer Trowbridge is abundantly able to defend himself, I leave the answer to the above extraordinary statements to him, and will simply remark, in passing, that I know from my own observation that the locking of the primaries can and does occur, either by accident or design, and that when it takes place it does not render flight impossible, as it affects only the extremities of the feathers. It is evident that Dr. Coues has not taken pains to inform himself in regard to the facts brought forward by Professor Trowbridge, otherwise he would not have uttered such dogmatic assertions.

Further on, Dr. Coues decapitates me much in the same way he does Professor Trowbridge; for he says, "The fixing of the wing of a mortally wounded bird in the manner described by Professor Newberry does not bear on the case. It is simply a muscular rigidity due to nervous shock, and of a part with the convulsive muscular action, which, under similar circumstances, results in the well-known 'towering' of hard-hit birds."

We have here other proof that Dr. Coues has not read all that has been said in this discussion: if he had done so, he would have seen that I did not claim that the automatic rigidity of the arm and fore-arm, the 'setting' of the wing, first described by Professor Wyman, had any thing whatever to do with the locking of the primaries. As was said in the discussion of Professor Trowbridge's paper before the Academy of Sciences, and reported in my former letter to *Science*, the spreading and folding, and, according to Professor Trowbridge, the locking of the primaries, are functions of the *manus*, and have nothing to do with the flexion and extension of the arm. The spread of the wings of the turkey-buzzard maintained after death, reported by me in my 'Notes on the Birds of Northern California and Oregon' (*Pacific Railroad Reports*, vol. vi. Zoölogy, p. 74), was certainly not a case of muscular spasm. My report of it will be found at the place cited, and is as follows: —

"For the purpose of examining this bird in California, to determine for myself its identity, or otherwise, with the turkey-buzzard of the East, I took occasion to shoot one which was flying over us in the upper part of the Sacramento valley. He made no motion indicating that he had been struck by my shot, but sailed on with widely expanded and motionless wings, as before. Gradually, however, he began to descend in wide and regular circles, till finally, without a wing-flap, he settled as lightly as a feather on the prairie, and remained motionless. I went to him, and found him resting in the grass, his wings still widely and evenly expanded, but the head drooping and life extinct. It was a male, large, in fine plumage, and apparently identical with ours; then, too late, I regretted that I had been the cause of a death so calm and dignified."

I have been shooting now for a great many years, have killed many thousands of birds, and ought to know what their behavior is when mortally wounded; yet I do not hesitate to say that the extension of the wings in this case and those reported by Dr. Storer was not due to muscular spasm, but to a locking of the wing-bones. Nor had the death of the turkey-buzzard, cited above, any thing whatever in common with the phenomena of 'towering,' as asserted by Dr. Coues. Towering is exhibited only by birds which are wounded in the head, and which, with confused intellects, fly up and up, perhaps till lost to view. I have reported one such case in my notes which is typical, and I here repeat my account of it to show that it was totally distinct from all wing-setting, spasmodic or articular.

"Once when collecting water-birds on San Pablo Bay, California, I shot a gull (*Larus Hermanni*), which fell, apparently dead, upon some rocks near me. When I stooped to pick it up, however, it flew swiftly away, and mounted in circles higher and higher until it disappeared."

The article by Bergmann in Müller's *Archiv für Anatomie und Physiologie* (1839) has no bearing upon the statements made by Professor Trowbridge or myself. It is true that Bergmann describes the sliding of the radius on the ulna, and in the discovery of this anatomical feature he antedates Wyman; but he makes no reference to the 'setting' of birds' wings, which was the special subject of Professor Wyman's note. All Bergmann says about the function of the anatomical peculiarity which he pointed out is, "that it is desirable that observations should be made (for which he had no opportunity) to determine whether it might not have efficiency in the soaring of rapacious birds or in the flight of those which must quickly change the direction of their flight."

In conclusion I will venture to suggest that neither Professor Trowbridge nor myself are such tyros in science as to warrant the didactic tone which Dr. Coues assumes. Professor Trowbridge needs no indorsement from me, but I venture to say that he is one of the most eminent engineers in the country, and that he has occupied himself for many years in the study of the mechanics of animal locomotion, upon which subject he is as well informed as any one living. As for myself, I was for many years as enthusiastic an ornithologist as Dr. Coues himself, and have shot over as much ground, and have perhaps killed as many birds. I was also educated as a physician, and, at the time I made the observations cited above, I was serving as naturalist and medical officer to a detachment of troops.

I would also call attention to the fact, that, for all the interesting information we now have in regard to the structure and functions of the wings of birds, we are indebted, not to ornithologists, but to

comparative anatomists: it is therefore quite possible that even Dr. Coues may learn something from one outside his profession.

J. S. NEWBERRY.

New York, Jan. 3.

THE communication of Mr. Elliott Coues in the last number of *Science*, on the mechanism of the flight of birds, renders a response from me, in the interest of science, indispensable. This is the more necessary on account of the unavoidable delay which has occurred in the publication of a paper sent by my son to the *Ornithologist and Oölogist*, and which will appear in the next number of that periodical, and also the delay in the publication of the paper read by me before the National Academy of Sciences, and which has given rise to comment and discussion, and is referred to in Mr. Coues's communication.

During the last autumn my son, C. C. Trowbridge, who is now a pupil in the Hopkins Grammar School at New Haven, Conn., and who has for several years devoted much of his leisure time to the collection and study of birds, brought to me a hawk which he had shot while it was soaring, and called my attention to the fact that the four outer primaries in each wing were interlocked; that part of each primary along which the lower margin was cut away lapping over or behind the succeeding primary, which was cut along its anterior or upper margin to permit of this interlocking and crossing of these feathers. This was the condition of the wings when he picked up the bird. The general appearances of the wings were so little altered from their ordinary aspects that the interlocking would ordinarily escape notice. My son suggested that this interlocking has the effect of relieving the muscular action required for the extension of the primaries during long flights, especially in soaring birds, and, further, that it might aid the bird in steering its way while soaring.

This discovery seemed to me of much interest; and after having assured myself by inquiries, and the examination of works on ornithology, that it had not been made by others, I concluded to bring the subject to the notice of the New York Academy of Sciences, and shortly afterwards read a paper in relation to the same before the National Academy of Sciences.

I supposed that all lovers of true science would welcome so interesting a discovery, even though it was made by a boy. Such, indeed, was the reception of the paper by all the naturalists present; Professor Marsh, Professor Newberry, and Professor Cope commending the paper, and Dr. Gill, who was not present, but to whom I had exhibited drawings of a wing, and explained the matter, giving encouraging assent to the novelty and importance of the discovery. I mention the names of these gentlemen, because I do not think they will ever have reason or cause to regret their favorable comments, nor to retract their opinions.

During many years' study of animal mechanics I have found no facts which exhibit more wonderfully and beautifully than those I have described, the mechanical adjustment of the organs of motion to the medium in which motion takes place, and to the conditions for which provision is made.

I have in my possession the wings of two large hawks (*Buteo lineatus* and *Buteo borealis*) in which the effects of the habitual interlocking of the four outer primaries has been to wear deep notches, and to produce permanent wrinkles, in the feathers at the point of crossing or overlapping. These I have shown to many scientific men without hearing a doubt expressed of the object or uses of the emarginate cuttings. These long primaries present a serious resistance, with a long leverage, when a bird is soaring, which would overtax the extensor muscles in long-continued soaring flights, if not relieved by the process of interlocking. That this interlocking does not impede flight, but in a wonderful and peculiar way aids the evolutions of the bird, is evident from the fact that by this interlocking a curvature is given to the anterior edge of the wing, which produces a warped surface, thus enabling the bird to have easy control of the wing with the least possible exertion. A perfectly flat, thin disk, in moving through air, is liable to be violently inverted, or turned broadside to the motion, by the slightest change of angle with the plane of motion. Every one has noticed this when a playing-card is seen to fall through the air. The edgewise position is one of extremely unstable equilibrium. This would be the condition of the outer part of the wing in soaring, were it not for the warped-surface form which, in the wings I have examined, is almost wholly maintained by the interlocking of the primaries, justifying my son's remark that this interlocking is an aid to steering, in soaring flight.

Mr. Coues, in his communication to *Science*, disposes of all this matter by a sententious dictum, which, from his extensive knowledge of ornithology, must be regarded as an extra-judicial opinion, pronounced with much regret, but with the severe force which science and truth demand, in the following words: —

"Much as I regret my absence on those occasions [the meetings at which the papers were read and discussed], I am still more sorry to be obliged to dissent without qualification from the position taken, . . . which is, to my knowledge, quite untenable. . . . With regard to the alleged locking of the primaries: 1. It does not take place; 2. Did it take place, flight would be impossible." And further, "It is fortunate that the mechanism of the wing does not permit the primaries to lock in the manner that has been supposed, for, if it did so, birds could not fly."

I am necessarily provoked, by these unexplained judgments, to test Mr. Coues's knowledge of the mechanism of the wing which "does not permit the primaries to be locked." I have found, by dissecting the wings of the hawks which I have referred to, that in these birds ten muscles are concerned in the movements of that part of the wing which corresponds to the human hand. Among these are three muscles, with their tendons, which have for their object solely the extension and flexion of the four or five outer primaries. The extensor muscles lie between the radius and ulna of the fore-arm, but the tendons run through the wrist-joint and along the hand to the joints of what corresponds in the human hand to the fore-finger, acting solely to extend the four or five primaries beyond any extension which they could otherwise have.

The flexor muscle lies in the hand, — a very small muscle, — with its tendon so attached that its only use is to flex the four or five primaries through the small angle by which they are extended by the opposing muscles just described. These muscles are not referred to, nor described, in Mr. Coues's admirable and voluminous work on ornithology, and I beg that he will inform the readers of *Science* where specific descriptions of these particular muscles, and their uses, can be found.

There are two other muscles whose tendons are so attached to the joints of the wrist, in the specimens I have, that when the wrist is extended or flexed by the larger extensors and flexors, a partial rotatory motion outwards and inwards may be given to the whole hand. May I ask Mr. Coues where I can find specific descriptions of these muscles, and their uses? These several muscles are principally concerned in the mechanism which does permit of the locking of the primaries.

Mr. Coues discusses another matter in his communication which has only a very general bearing on this question of the primaries. It is the automatic or concomitant extension and flexure of the wrist in birds when the elbow is extended or flexed. In the specimens which I have examined, I have found an inelastic tendon, without a muscle attached, fastened at one end to the humerus at the elbow, and at the other to the hand at the wrist, which is an essential feature in this purely kinematic combination. Moreover, this tendon, or string, plays another important part in acting as a string to the bow of the ulna, and taking the strain which might break the ulna, when the bird strikes the air strongly, but for this remarkable support. This is not referred to in Mr. Coues's work, and I would ask him where I may find its description.

Finally, will Mr. Coues explain *why* birds cannot fly when a few inches in length of the outer primaries lap over and behind others? Mechanically this makes a very strong wing, admirably adapted to soaring flight, for which it is evidently intended; and in one instance, at least, which I have given, the bird did apparently fly very well and very naturally with its primaries thus interlocked.

Moreover, from my own experiments with wings, both before dissection and after the muscles and tendons have been exposed, so that they might be operated by hand, I am convinced that the interlocking of the primaries is a simple and easy operation, entirely under control of the bird, and with many birds is habitual.

W. P. TROWBRIDGE.

New York, Jan. 3.

Eskimo and the Indian.

THE criticisms of Mr. Chamberlain's letter (*Science*, Dec. 2) by Dr. Boas and Mr. Murdoch are sound, forcible, and instructive; but these critics have confined their strictures wholly to the Eskimoan words. So, using the alphabet adopted by the Bureau of Ethnology for recording Indian languages, I will point out some errors made by Mr. Chamberlain in the words of his comparative list taken from the Iroquoian languages.

After making due allowance for the rude and imperfect 'orthography' of the words, it is necessary to say that *ata* ('father') and *ekening* ('woman') are not Tuscaroran terms; that *nup* ('die') and *nibey* ('water') are not Mohawk words; that *aitaa* ('father') is not Huron: these vocables, having these forms and with these meanings, are not Iroquoian.

(1) *kwe'-nis*, and not *quennies*, is the correct form of this term for 'copper.' It is evidently the word 'penny' or 'pence' (possibly *penning* or *peningens*), adopted by the Iroquois, and adapted to their own peculiar utterance. In earlier times they most invariably substituted *kw* for *p* or *b*, because these sounds did not occur in their speech.

(2) *kă-nă'-tcyă'* is the proper form of *kanadzia*, and, being predicative, it signifies 'it is copper,' and not simply 'copper;' it also means 'it is a pot or kettle,' and is more frequently used in this latter sense. Its derivation is not clear, but, in accordance with the genius of Iroquoian speech, it presupposes the nominal or substantive form, *o-nă'-tcyă';* this, in turn, points to an earlier *o-nă-tcyo''-kwe*, — a form still extant in some of the cognate languages, and which form is evidently from the predicative *ye-nă-tcyo''-kwă'* ('one cooks rice (wheat) by which'), undoubtedly referring to the cone-bottomed earthen 'pots' or 'kettles' so used. The circumstance that unburnished copper resembles very much these clay 'pots' in color would quite naturally serve as a distinctive characteristic by which to describe this metal. *Kă-nă'-tcyă'* as a predicative signifies either 'pot' or 'copper,' but as a substantive, only 'pot,' which is probably its oldest meaning.

Mr. Chamberlain compares the preceding two words with the Eskimoan *kannooyak* ('copper'). One of the two is clearly of European origin, and the other is possibly, but not probably, related to the Eskimoan term.

(3) *e'-hne'-kĕñ* is the proper orthography of *ehneken*. It is a derivative term denoting 'above,' 'on the surface.' Its probable original signification is 'sun-ward,' 'sun-side,' or 'toward-sun.' It certainly never meant 'sky' in Iroquoian speech; but the Unalashkan *innyak* with which it is compared means both 'sky' and 'above' in the list.

(4) *o-nĕñ'-yă'*, and not *onna*, is the proper Iroquoian word for 'bone.' The Eskimoan *hrownik* ('bone') has clearly no 'fortuitous coincidence' of sound with it.

(5) *he-'gĕñ'-hă*, and not *haenyeha*, is the proper form of this Iroquoian expression. It signifies 'my younger brother' (literally, 'my brother small'), and not simply 'brother.'

(6) *tcyă'-tă'-te-kĕñ*, and not *jattatege*, is the true form of this vocable: it means 'ye two are brothers to each other,' and not 'brother' alone. The Eskimoan *anayoa* ('his elder brother'), *anaga*, and *agituda* have clearly no evident similarity of sound or meaning with the two preceding Iroquoian words, *he-'gĕñ'-hă* and *tcyă-tă'-te-keñ*.

(7) *she-yĕn'-hă*, and not *cheahhah*, represents the orthoepy of this predicative term, which means 'thy daughter,' and not simply 'child.' Literally it signifies 'thou one hast small.' The Eskimoan word *iyaye* ('child') has no apparent affinity here.

(8) *e-nĭ''-se-ră'*, and neither *eghnisera* nor *ennisera*, is the proper form of this word, meaning 'day,' a form used mainly in composition. It is a derivative form of the word *ĕñ-tă'* or *e-nĭ''-tă'* ('day,' originally 'sun'). The Eskimoan *anyark* evidently means 'a long day,' and not simply 'day.' No similarity of sound or meaning appears here.

(9) *koñ'-nĭ's* (meaning 'I make, build, or render it,' and not simply 'do') is a better form of *konnis*. *K* for *ka-* ('he — it'), *-oñ-nĭ'-* ('make,' 'build,' or 'render'), *-s* (terminative sign of customary action), — this is the etymology of the word, which has no similarity of sound or meaning with the Eskimoan *tcheneyoaq* ('he works').

(10) *shĕn-tĕ'n'-kyĕ*, and not *suntunke*, represents the proper pronunciation of this word: it means 'on or against thy ear,' and not simply 'ear;' the initial *s-* is the sign of the second person possessive, *-'kye* is the locative, and *-hĕn-t-* is the noun stem or root. The Tchuktschi *tchintak*, or correctly *siuta* ('his ear'), apparently has no affinity with this word.

(11) *e-nyĕñ'-kyĕ*, and not *ayinga*, misquoted from *eyingia*, is the correct form of this word, which signifies 'on or against one's hand,' and not 'finger,' as does the Tchuktschi *aihanka*, with which it is questionably compared.

(12) *yu''-năks*, and not *yoneks*, means 'it is burning,' and not simply 'fire,' as in the list: *yu-* ('it'), *-'năk-* ('to burn'), *-s* (the terminative sign of customary action). The Eskimoan *oonoktook* ('fire' or 'to burn') has but a doubtful claim to relationship with this word.

(13) *o-sĭ''-tă'*, and neither *achita* (Huron), nor *ochaita* (Onondaga), is the correct form of this vocable, meaning 'foot.' The stem or root of the word is *-sĭ'-t-*, a stem that never meant 'hand.' The Eskimoan *etscheak* or *arkseit* ('foot' or 'hand') has certainly no evident affinity with this word.

(14) *yo-yă'-nĕ-re'*, and not *ioyanere*, is the true form of this term or expression, which signifies 'it is good' affirmatively: thus, *yo-* ('it'), *-yă-nĕ-r-* ('the good,' 'the right,' or 'the noble'), *-re'* ('to have or possess'). The Eskimoan *ayunitork* or *ayunitsoq* means 'not bad,' and so 'good' negatively. These two words evidently have no affinity nor a common origin.

(15) *os-o''-tă'*, and not *chotta*, is the proper form of this word, denoting 'hand.' With this meaning it is common to only two of the Iroquoian languages. Originally it meant 'finger,' signifying literally 'hand-protruding-thing.'

(16) The orthography of *noatsshera* is so uncouth that it is very difficult to discover its meaning. It does not mean 'head,' but 'hat' or 'scalp-covering.' It is properly written *o-no-he'-tcră*, which form has no relation whatever to Tchuktschi *naschko* ('head').

(17) *o'-'skwă'*, and not *hechkwaa*, is the proper orthography of this term for 'lip.' It bears no resemblance to the Eskimoan word *kakkiviar* ('lip'), with which it is compared.

(18) *e-'nĭ'-hă*, and not *enihah* nor *aneehah*, is the proper spelling of this word, which means 'one is male,' but never 'man.'

(19) *oñ'-kwe*, but not *onquich*, is the correct form of this term, which denotes 'homo,' 'man,' 'a human being,' but never 'male,' to distinguish sex. This word, and *e-'nĭ'-hă* above, have no root in common: so, having no literal meaning common to both of them, they should not be compared with one and the same word. The Eskimoan words *angut* and *innuk* ('man') are not related, and why compare them with two Iroquoian terms (*ĕ-'nĭ'-hă* and *oñ'-kwe*) likewise unrelated to each other?

(20) *ĕn'-nĕn'* represents the true form of *anehah* (Huron), *eanuh* (Tuscarora), *ana* (Nottoway), and means 'my mother,' not simply 'mother.' The root of the word is *-ĕn-*, which signifies 'mother.' It is my belief that it is related to *-oñ-nĭ'-* ('to make or produce,' 'to build or render').

(21) *o-nyoñ'-să'*, not *yaunga*, is the true form of this Iroquoian word for 'nose.' It has no apparent affinity to Tchuktschi *chinga* ('nose').

(22) *kwĕn-tă'-ĕn*, but not *quechtaha*, is the correct form of this Seneca-Iroquoian word, meaning 'red.' It is compound, and evidently signified 'it is blood-marked,' and so 'it is red.' There is an evident metathesis of the first and second syllables. The Tchuktschi *kawachtuk* ('red') clearly has no affinity whatever to this word.

(23) *ă-wĕn-nă''-să'* and *ă-wĕn-tă''-sĕ* are the forms of *ennasa* ('tongue') found among the Iroquoian languages. They do not, however, resemble the Unalashkan *ahnak* ('tongue').

(24) *o-nye'-yă'* and *o-ni-ye-te* are the true Mohawk forms of *ouniyeghte* ('snow'); *o-ni'-yă* is the proper Seneca form of *onyeiak* (also 'snow'): these words have no apparent relation to Tchuktschi *annu* or *annju* of the same meaning.

(25) *so'-rak*, not *soluck*, is the true Mohawk word for 'duck.' The Eskimoan word for 'duck' is *tchorlerk*.

(26) *o'-she*, and not *oxhey*, is the correct form in Huron of this Iroquoian word for 'winter' or 'year.' Its stem is *-sh-*, and means 'snow.' The Eskimoan *ukshiok* and *uktschuk* have no apparent affinity or relationship with this word.

Thus, in comparing thirty different words taken from the several Iroquoian languages, there is scarcely a single instance in which Mr. Chamberlain has not misapprehended the true sound and real meaning of the words.

Before an effective or satisfactory comparison between the words of two languages, or of two families of languages, can be made, the investigator should possess at least an elementary knowledge of both, a knowledge of their rules of etymology and syntax, and of their laws of vocalic and consonantic change. This is especially true with reference to the languages of the Iroquoian peoples. These tongues are among the most difficult of Indian languages to investigate and to understand.

To a want of knowledge of these facts, and to the use of faulty vocabularies, are evidently due Mr. Chamberlain's errors. An attempt to establish the affinity and common origin of two languages upon material so faulty as that criticised is scarcely likely to be successful. J. N. B. HEWITT.

Bureau of Ethnology, Washington, D.C., Dec. 26.

The Study of Languages.

YOUR correspondent, H. L. E., asks in the last issue of *Science* whether there is any practical method of learning to read a language without the use of a dictionary. The present writer has learned to read readily two languages without the use of either dictionary or grammar, and believes his method not only possible, but the better way, when a knowledge of the language, not its grammar, is the one desired. His plan has been to begin with some easy author, and follow its text closely while some one reads aloud an English or some other familiar translation. By following such a plan through a dozen or more books, one may then venture on some simple author, dispensing with both dictionary and translation so far as possible, and learning the meanings of the new words, as they appear, from the context. After having read twenty or thirty novels or similar works in this way, he should begin the study of the grammar, and will then be surprised to find that conjugations and declensions are no longer a task. After one has learned a language, a dictionary is very useful; but he certainly can never get a thorough and exact knowledge of the meanings of words from English synonymes. W.

New Haven, Dec. 30.

Conspiracy of Silence.

THE following statement, made by one of your correspondents (*Science*, x. 309) — "But a general conspiracy among men of science to suppress views because they are new and unacceptable to old fogies, is impossible; and your correspondent and the Duke of Argyll must certainly know that fact, and it will remain a fact, in spite of any number of instances of special local repression that can be cited" — is a logical curiosity. Whether or not the general conspiracy exists can only be known by examining the local action in special cases which may arise; but we are told, that, whatever be the result of this examination, we must recognize the impossibility of such a conspiracy. This is decidedly a new process of scientific demonstration. Old Poz, who remarked, "I've said it, and that's enough to convince me," was accustomed to reason in this manner.

The same correspondent states, speaking of Mr. Bonney, "What he meant in his rebuke of the Duke of Argyll is evident: he meant that any one man of science not engaged in a given special line of research can not and dares not make up his own mind as to the validity of one of two opposing theories until those others who have that special line of research in hand have practically reached some consent on the subject."

This is the true ecclesiastical method, to which Mr. Bonney objected. It is the method of the child in the song, who says, —

"I believe it, for my mother told me so."

It is the method of the man who has a profound reverence for authority, so well pictured by Thackeray: —

"So, as he had nothing to say in reply, he began to be immensely interested in the furniture round about him, and to praise Lady Clavering's taste with all his might.

"'Me, don't praise me,' said honest Lady Clavering, 'it's all the upholsterer's doings and Captain Strong's, they did it all while we was at the Park — and — and — Lady Rockminster has been here and says the salongs are very well,' said Lady Clavering with an air and tone of great deference.

"'My cousin Laura has been studying with her,' said Pen.

"'It's not the dowager: it is *the* Lady Rockminster.'

"'Indeed!' cried Major Pendennis, when he heard this great name of fashion, 'if you have her ladyship's approval, Lady Clavering, you cannot be far wrong. Lady Rockminster, I should say, Arthur, is the very centre of the circle of fashion and taste. The rooms *are* beautiful, indeed!' and the major's voice hushed as he spoke of this great lady, and he looked round and surveyed the apartments awfully and respectfully, as if he had been at church."

It may be that the views imputed by Mr. Lesley to Mr. Bonney are correct, but this would not be suspected from the latter's published words: and it looks as if Mr. Bonney's defender, in his zeal, has given away Mr. Bonney's case, and the scientist's case in general, more completely even than was done by Mr. Bonney himself.

RICHARD H. BUEL.

New York, Dec. 30.

Color and Other Associations.

IN a note on color and other associations, which I wrote, and which was printed in *Science* (vi. 1885, p. 242), I gave the colors which my daughter Mildred (then a child eight years old) associated with the days of the week, with the numerals 1–10, and with the letters of the alphabet in 1882. I stated that I found the same colors associated with the same forms in 1885. I have lately questioned her again, and I find the same colors are still associated with the same forms in nearly every case. Saturday's color has changed from pure white to cream color; F has changed from black to brown; Q, which had no certain color, is now called purple; X and Y, which had not much color, are now called red and cream color (Q, X, and Y are now more frequently in use than then); 8, which was white, is now called cream color (a similar change to that of Saturday); and 9, which was called 'greenish?' is now called blue. With these few exceptions, the same colors have been constantly associated with the same days, numerals, and letters from 1882 to 1888, — six years. This case appears to me now, as formerly, to deserve record in connection with the observations of Galton and others on the subject.

EDWARD S. HOLDEN.

Berkeley, Cal., Dec. 20.

Thomas Braidwood and the Deaf-Mutes.

IN a footnote to a page of Sir Walter Scott's 'Heart of Mid-Lothian,' I read, "'Dumbiedikes' is really the name of the house bordering on the King's Park (Edinburgh), so called because the late Mr. Braidwood, an instructor of the deaf-and-dumb, resided there with his pupils."

Now, I happen to know that Thomas Braidwood sold his estate (that goes by the name of our family, and is situated next to the Duke of Hamilton's, some twenty miles beyond Glasgow) in order to use the proceeds to start his institution for educating the deaf-and-dumb; and if Professor Bell, in his address at the Gallaudet anniversary, a notice of which is published in *Science* of Dec. 23, meant it as a reproach to the memory of Mr. Braidwood, when he says the school "was a money-making institution," and that its principal "had bound all his teachers under a heavy fine not to reveal his methods to any one," it may be pertinent to ask if, under the circumstances, it was not only prudent, but a duty of Mr. Braidwood, to make his institution pay its own way. His all was involved in it; and, had he not used what some people would call a necessary precaution, his school might have perished for want of funds, and himself been impoverished. At all events, that is the view his relations take of the matter.

And when one reviews the dreary centuries preceding, when every now and again some gentle soul proposed to educate the deaf-and-dumb only for it to drop out of thought again, perhaps it would be best to guard with caution the acts of him who staked his entire wealth in the venture, and spent forty-six years of life in establishing as a living fact what was but as a grand dream for centuries.

THOMAS W. BRAIDWOOD.

Vineland, N.J., Dec. 29.